the Birmingham and Midland Institute—is to be congratulated on the good work he has done in this connection. The book is illustrated with 72 figures, which agree with the simplicity and clearness of the diction, and questions are found at the end of each chapter, which have been well prepared to test the learner's apprehension of its contents. We are pleased to be able to recommend this little work, as a foundation for the study of the metallurgy of iron and steel.

OUR BOOK SHELF.

On the Creation and Physical Structure of the Earth. By J. T. Harrison, F.G.S., M.Inst.C.E. (London: Longmans, 1889.)

THIS book brings to mind one of the most winning of the vagaries of childhood. A bright child of an inquiring turn will sometimes sit with comical sedateness listening to the talk of its elders. It may afterwards be overheard repeating to one of its playmates, or to some lucky adult who has the knack of winning its confidence, such detached scraps of the conversation as have found a resting-place in its little brain; and, conscious even at its early age of the necessity of some continuity in a narrative, filling up the gaps with inventions or criticisms of its own, charming every way, but mainly on account of their utter want of connection with the subject of the conversation which it is attempting to report. So our author has listened to the teaching of many geologists, and has culled many detached passages from their writings: these he repeats to the world in a book, printing between them comments and lucubrations of his own, about as innocent and as little apposite as the child's prattle—hardly so amusing, however. The following passage is a fair sample of the writer's own share in the book. "The termination of the Secondary Period, which introduced these altered conditions of the surface of the northern hemisphere, was really the commencement of what is called the Glacial epoch in Europe. We have noted signs of glaciation during the deposition of the upper chalk in India and North America, but now the conditions which induced that glaciation are extended in such a manner as to unite these districts, and produce that enormous accumulation of snow and ice at the North Pole, the weight of which in the Miocene epoch depressed the crust in that region and upheaved the mighty mountain ranges to which I have just referred."

The book bristles with cataclysms and catastrophes. The shifting of a thin crust on an internal nucleus which it does not fit, and incessant protrusions of granite, are invoked to account for phenomena which every-day people still persist in thinking are satisfactorily explained by every-day causes. But the author is one born out of due time—two centuries too late. How he and Burnet would have enjoyed a crack together! But there is this to be said, the "Sacred Theory of the Earth" is Burnet's own: the staple of the present work consists of extracts from the works of others. The mottoes are verses from the first chapter of Genesis, but their relevancy to the subject-matter of the chapters which they head is not obvious.

A. H. G.

Through Atolls and Islands in the Great South Sea. By F. J. Moss. (London: Sampson Low, 1889.)

MR. MOSS—a member of the House of Representatives, New Zealand—started from Auckland, in September 1886, in the schooner *Buster*, for a voyage among the islands and islets of "the outer lagoon world." He was absent seven months, and during that period he crossed the equator six times, and visited more than forty islands among the least frequented groups. In the present volume he sums up the impressions produced upon him by what he saw and heard in the course of his voyage. Mr. Moss, in dealing with matters which really interest him, shows that he is an accurate observer and a man of sound judgment. His style, although plain and unpretending, is well fitted for the task he has fulfilled. The best parts of the book are those in which he tries to convey some idea of the daily life led by those natives whose customs he had an opportunity of studying. He appreciates warmly some aspects of the various Polynesian types of character, but thinks that the people are likely to degenerate rapidly, unless they can be provided with a better class of native teachers than most of those to whom the duty of guiding them is now intrusted. What is needed, he thinks, is, that the islanders shall have in their work and in their amusements freer scope for the imaginative powers with which they are endowed, and the exercise of which is too often foolishly discouraged. Everything Mr. Moss has to say on this subject deserves the serious consideration of those to whom his warnings and counsels are either directly or indirectly addressed.

LETTERS TO THE EDITOR.

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Who Discovered the Teeth in Ornithorhynchus?

IN NATURE of November 14 (p. 31), Profs. Flower and Latter criticise my note which appeared the week previous (November 7, p. 11), concerning the discovery of teeth in the young *Ornithorhynchus.* They promptly dismiss my claim that Sir Everard Home discovered the teeth of the young *Ornithorhynchus*, by stating that the structures described and figured by Sir Everard are the well-known cornules of the adult animal.

If they will take the trouble to turn to the plate cited by me—namely, Plate lix. of the second volume of Home's "Lectures," 1814—and will read the accompanying explanation, they will see that Home was familiar with the teeth of both the young and the old animal.

For the benefit of those who may not have access to Home's "Lectures," I here reproduce outline tracings of two of his figures. Plate lix. Fig. 2, shows the teeth of the *young Ornithorhynchus*—the "first set," as Home says, "to show that there are two grinding teeth on each side." The next figure is a similar tracing from the succeeding plate in Home's "Lectures" (Plate lx.), which represents, to again use Home's words, "the under jaw of the full-grown *Ornithorhynchus paradoxus*, to show that there is only one grinder on each side." Both of these figures are natural size.

In the face of these *facts*, further comment seems unnecessary.

I admit, of course, that Home did not discover the chemical composition of the teeth of the young animal—this was Poulton's discovery.

C. HART MERRIAM.

Washington, D.C., November 30.

[We do not reproduce the outlines sent, as anyone interested in the subject may see the originals, not only in Home's "Comparative Anatomy," but in the Philosophical Transactions, where they first appeared.—ED. NATURE.]

I SHOULD be very sorry to deny the credit of any discovery to Sir Everard Home, or anyone else, if any evidence could be shown of its having been made. Of the figures cited by Dr. Hart Merriam, that of the younger animal seems (as far as can be judged from the roughly executed engraving, with the assistance of the descriptive text) to represent the horny plates, showing the hollows from which the true teeth have recently fallen; that of the old specimen, the same plates after they are fully grown, and their surfaces worn down by attrition. This difference led Home to conjecture that these plates were changed during the growth of the animal—a view which was corrected by Owen ("Comp. Anat. of Vertebrates," vol. iii. p. 272), by the statement

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that "each division or tubercle of the [horny] molar is separately developed, and they become confluent in the course of growth." By the way, no one can have been better acquainted with the work of Home than his successor in the Hunterian Chair, Sir Richard Owen; and yet, in his numerous references to this subject (Art. "Monotremata," "Cyclop. Anat. and Physiology"; "Odontography"; "Comp. Anat. of Vertebrates," &c.), no trace is shown of any knowledge of a discovery which could not have failed to have interested him, if it had been made before his time.

If a cursory perusal of Sir Everard Home's first account of the mouth of the Ornithorhynchus (in the Philosophical Transactions for 1800), or any interpretation placed upon his figures, might lead anyone to infer, with Dr. Merriam, that the real teeth of the young animal had been discovered at that time, the best possible authority may be conclusively cited against such an idea, no other than that of Home himself, who, in his later description of the same specimen ("Lectures on Comparative Anatomy," 1814), describes the organs in question as "the first set of *cuticular teeth*"—an expression quite incompatible with their being the teeth described by Mr. Poulton and Mr. Oldfield Thomas. It really seems superfluous to have to remind a zoologist of such high repute as Dr. Hart Merriam that the difference between teeth with the structure and mode of growth which characterize these organs in the Mammalia generally, and the horny epithelial plates of Ornithorhynchus, is not merely one of "chemical composition." W. H. FLOWER.

The Pigment of the Touraco and the Tree Porcupine.

ATTENTION has been lately again directed to the red pigment in the wing feathers of the touraco, which has been stated by several observers to be soluble in pure water. Prof. Church, who was the first to experiment upon this pigment (*The Student*, vol. i., 1868; *Phil. Trans.*, 1869), quotes Mr. Tegetmeier and others, to the effect that this pigment can be washed out of the feathers by water. Later, M. Verreaux (Proc. Zool. Soc., 1871) confirmed these statements from his own experiments while travelling in South Africa; attempting to catch one of these birds whose feathers were sodden with rain, he found that the colour stained his hands "blood-red." A few years ago Prof. Krukenberg ("Vergl. Phys. Studien") took up the study of turacin—as Prof. Church termed the pigment—and added some details of importance to Prof. Church's account; Krukenberg, however, contradicted certain of the statements quoted by Church with reference to the solubility of turacin in pure water, remarking that the pigment in the dead bird is insoluble in water. A writer in the *Standard* of October 17 is able "partially to confirm" the assertion that turacin is soluble in pure water. Seeing that there is some conflict of opinion with regard to this matter, I think it worth while to state that I found it quite easy to extract with tap water (warm) some of the pigment from a spirit-preserved specimen of the bird; only a very small amount could be extracted in this way, and the feathers were not perceptibly decolorized even after remaining in the water for a fortnight. I also experimented upon a feather just shed from one of the specimens now in the Zoological Society's Gardens; this was steeped in water for some time without any effect being visible, but after a period of two days the water became stained a very faint pink.

The touraco, however, is not a unique instance of a terrestrial animal with an external colouring matter soluble in water. I am not aware whether other cases have been recorded, but I find a pigment of a similar kind in a South American tree porcupine (*Sphingurus villosus*).

This porcupine has bright yellow spines which are for the most part concealed by abundant long hair. The spines themselves are parti-coloured, the greater part being tinged with a vivid yellow; the tip is blackish-brown. I was unable to extract this pigment with chloroform, or with absolute alcohol even when heated; like so many other colouring substances which are insoluble in these fluids, the pigment could be extracted by potash or ammonia; I found also that tap water, warm or cold, dissolved out the yellow colour; the action was slower than when the water was first rendered alkaline by the addition of ammonia, but, unlike the touraco, the pigment was nearly, if not quite, as completely dissolved. The skin, from which the spines were taken, was a dried skin of an animal recently living in the Zoological Society's Gardens; it had not been preserved in alcohol or treated in any way which might lead to the supposition that the pigment was chemically altered. There is, therefore, a considerable probability that in the living animal the pigment is also soluble in water. I believe that this yellow pigment is undescribed, but I have not yet completed my study of it; in any case, it is not zoofulvin or picifulvin, or any "lipochrome." FRANK E. BEDDARD.

Exact Thermometry.

IN the account which Prof. Mills has given (NATURE, December 5, p. 100) of M. Guillaume's "Traité pratique de la Thermométrie de précision," the permanent ascent of the zero-point of a mercurial thermometer, after prolonged heating to a high temperature, is stated to be due to compression of the bulb—rendered more plastic by the high temperature—by the external atmospheric pressure.

The constant slow rise of the zero-point of a thermometer at the ordinary temperature is mentioned by Prof. Mills; and the late Dr. Joule's observation of this change in a thermometer during twenty-seven years is specially alluded to. It may, I imagine, be taken for granted that after the lapse of a sufficient length of time—possibly many centuries—a final state of equilibrium would be attained; and it has always appeared to me that the effect of heating the thermometer to a high temperature is simply to increase the rate at which this final state is approached. It is my impression that, owing to the more rapid cooling of the outer parts of the bulb after it has been blown, the inner parts are in a state of tension, as, to a very exaggerated degree, in the Prince Rupert's drops; and that it is the gradual equalization of the tension throughout the glass that causes the contraction; in other words, that the process is one of slow annealing.

This explanation appears to be supported by the facts—(1) that when a thermometer is exposed for a long time to a high temperature, the zero-point rises rapidly at first, then more and more slowly, and finally becomes constant or nearly so; (2) that the higher the temperature the more rapidly is this state of equilibrium attained. I do not know of any experimental evidence that the rate of ascent is influenced by changes of external pressure, and it seemed to be desirable to test the point.

In order to do this I have exposed three thermometers, A, B, and C, constructed by the same maker and of the same kind of glass, to a temperature of about 280° for several days in the same vapour-bath, under the following conditions:—The thermometers were all placed in glass tubes closed at the bottom (C being suspended from above), and the tubes were heated by the vapour of boiling bromonaphthalene. One of the tubes—that containing thermometer C—was exhausted so as to reduce the external pressure on the bulb to zero; the others were open to the air. In thermometer A there was a vacuum over the mercury, but air was admitted into B and C to increase the internal pressure. Consequently, the bulb of A was exposed to a resultant external pressure equal to the difference between the barometric pressure and that of the column of mercury in the stem of the thermometer; the internal and external pressures on the bulb of B were approximately equal; lastly, the internal pressure on the bulb of C was the sum of the pressures of the column of mercury in the stem and of the air above it, while the external pressure was zero.

The following results were obtained:—

	A.	Rise.	B.	Rise.	C.	Rise.
Zero before heating ...	0·15		0·10		−0·10	
		0·35		0·25		0·40
After 2 hours' heating ...	0·50		0·35		0·30	
		0·80		0·75		0·80
After an additional 5½ hours' heating	1·30		1·10		1·10	
Total rise of zero-point...		1·15		1·00		1·20

The thermometers were heated until 5 p.m. each day, and the zero-points read on the following morning.

If the diminution of volume of the thermometer bulb, usually observed, were due to external pressure, the zero-point of A should have risen, that of B should have remained nearly stationary, while that of C should have fallen. Instead of this, however, the zero-points of all three thermometers rose at nearly the same rate; therefore the yielding of the bulbs to pressure, owing to the plasticity of the glass, if it occurred at all, had no sensible effect on the result. SYDNEY YOUNG.

University College, Bristol, December 12.

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